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Crime, compulsory schooling laws and education

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ABSTRACT

Do compulsory schooling laws reduce crime? Previous evidence for the US from the 1960s and 1970s suggests they do, primarily working through their effect on educational attainment to generate a causal impact on crime. In this paper, we consider whether more recent experience replicates this. There are two key findings. First, there is a strong and consistent negative effect on crime from stricter compulsory schooling laws. Second, there is a weaker and sometimes non-existent link between such laws and educational attainment. As a result, credible causal estimates of the education–crime relationship cannot in general be identified for the more recent period, though they can for some groups with lower education levels (in particular, for blacks).

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1. Introduction

Few doubt that educational attainment and crime are related. Two examples illustrate this point. In the United States, 41% of inmates in prisons and jails in 1997 had not completed high school, compared to only 18% of the general population. Second, a survey of newly sentenced prisoners in the UK in 2005/2006 showed that 47% had no educational qualifications, compared to 15% for the general population (Hopkins, 2012). What is less clear is whether this link represents a causal relationship running from educational attainment to criminal behaviour, or whether it merely reflects a whole set of personal characteristics associated with lower education levels that denote those on the margins of society. This is important for policymakers as they assess the potential social returns to education.

Significant research progress has been made on this question. Our overall reading is that there is quite strong evi-

dence of the existence of a causal effect of education on crime, which is usually identified by using changes in compulsory schooling laws to generate exogenous variation in educational attainment.¹ Having said that, causal evidence using these laws that relates to recent experience is not available. For example, the most cited paper is that of Lochner and Moretti (2004) who study incarceration data that end in 1980, and arrest data that end in 1990. Anderson (2014) does study more recent arrest data for juveniles, but with a focus on whether changes in dropout age matter for crime, but not on education.

This paper adds to this literature in two key ways. First, we focus on both crime and education more recently. This is important since both crime and education patterns are different post-1980 than before. On the former, at least in the United States, the patterns of crime and incarceration are very different before and after 1980. The substantial rise in the prison population was predominantly in the later period, while total

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¹ See also the review of the empirical literature on education and youth crime by Rud et al. (2013).

property crime has fallen substantially. On the latter, education levels are higher (in part reflecting longer run trends) and also part of the reason for the focus on the earlier period is that many of the major changes to compulsory schooling laws occurred prior to 1980, which is the variation in the data used to identify the exogenous movement in schooling (see Stephens & Yang, 2014). However we show that there is still substantial variation in these laws over the 1980–2010 period – though we emphasise that the group of students affected by the laws may be very different from those affected by earlier changes (i.e. the composition of the compliers may not be stable over time). This change in composition then may generate different effects on both education and crime.

A second feature is that we exploit a new set of geographical groupings for the continental United States that allows us to consistently combine data on arrests from the FBI Uniform Crime Reports (UCR) with micro-data from the US Census. One issue with the micro-data from the Census is that it has traditionally been hard to construct any consistent geography below the state-level that can be identified across successive Censuses. So for example, county-level data cannot be used because only the larger counties are uniquely identified in the micro-data. Autor and Dorn (2013) have painstakingly generated commuting zone areas for the US that can be consistently identified from the 1970 Census onwards. We have been able to match these areas to UCR reporting agencies, though the arrest data are only available on a consistent basis from 1974 onward, which restricts us to starting with the 1980 Census – so we study the 1980–2010 time period.

The rest of the paper is organised as follows. In Section 2 we outline an empirical framework for thinking about the empirics of education and crime, and briefly review the extant evidence on the causal relationship between education and crime. Section 3 discusses the data we use in this paper and the empirical methodology. In Section 4 we present a range of evidence on the reduced-form relationship between crime and compulsory schooling laws. We show that there is a consistently strong negative link between crime, measured either by arrests or incarceration, and schooling laws. We also present evidence to show that some of this effect appears to be a result of incapacitation, but this cannot explain all of the effect. In Section 5 we assess whether these results allow us to identify a causal effect of education on crime. The evidence turns out to be mixed. For whites, who on average have higher education levels, our answer is no. This is because of the difficulty in generating a strong and coherent first-stage for schooling from the school leaving laws, in contrast to the earlier evidence from the 1960s and 1970s. In contrast, for blacks we can still estimate a causal crime reducing effect of education – though even here the power of the first-stage is weaker than in the past. The conclusions are in Section 6.

2. A framework and previous findings

2.1. Empirical set up

To motivate our analysis, consider the basic relationship between crime and schooling:

$$\text{Crime}_i = \beta \text{Schooling}_i + \gamma X_i + \varepsilon_i \quad (1)$$

where X is a set of control variables and ε_i an error term. At the moment (1) is pitched as an individual-level (denoted by i) regression of crime on schooling that holds constant the factors included in X . We will generalise this in several dimensions in our actual empirical work, as detailed below, but the relatively simple formulation in (1) serves its purpose for motivating our analysis.

Schooling is unlikely to be exogenous in (1). Thus to generate an estimate of β that yields a causal impact of schooling on crime we require an instrument that satisfies the usual conditions. In this paper, as with much of the literature, alternative measures of compulsory schooling laws (CSL) will serve as the instrument. Therefore underpinning an instrumental variable/two stage least squares (IV/2SLS) estimation of (1) we have reduced-form equations for crime and schooling (the first stage) which are given respectively as:

$$\text{Crime}_i = \theta_1 \text{CSL}_i + \pi_1 X_i + \varepsilon_{1i} \quad (2)$$

$$\text{Schooling}_i = \theta_2 \text{CSL}_i + \pi_2 X_i + \varepsilon_{2i} \quad (3)$$

where the IV (2SLS) estimate of the coefficient on the schooling variable in (1) is the ratio of the reduced-form coefficients in (2) and (3), so that $\beta = \theta_1/\theta_2$. In general, we would expect $\beta < 0$ since higher educational attainment is expected to reduce crime. This works through the two reduced-forms as θ_2 will be positive if schooling laws increase schooling, and the schooling laws reduce crime so that $\theta_1 < 0$.

In understanding a causal crime reducing impact of education in this set up, Lochner and Moretti (2004) note there is no simple relationship between the strength of the reduced-form for crime in (2) and the strength of the IV estimate in (1). On the former, θ_1 can be negative for at least two reasons. First, if there is indeed a causal link between education and crime and schooling laws increase education, then the coefficient on CSL in the reduced-form for crime will pick up this effect. An alternative, though in no sense mutually exclusive, possibility is that such laws can directly reduce crime, over and above their effect on educational attainment.

If there is a direct impact of school leaving laws on crime, to the extent that such laws force juveniles to be in a supervised environment rather than roaming the streets, then this can be interpreted as a straightforward incapacitation effect that reduces crime.² There is a body of evidence to support this which uses plausibly exogenous changes in the length of the school day or exploits random days in which schools do not open to identify incapacitation effects (Jacob & Lefgren, 2003; Luallen, 2006).³ However, we also know that criminal behaviour peaks in the late teenage years. If the incapacitation effect reduces criminal activity at these crucial ages, it may in addition generate a persistently lower

² Other types of incapacitation effects that have been studied include Black et al.'s (2008) work on teenage births and Galiani et al. (2011) on conscription and crime.

³ Earlier (non-causal) estimates of incapacitation (time spent in school) effects were presented in Gottfredson (1985), Farrington et al. (1986) and Witte and Tauchen (1994). Hjalmarsson (2008) looked at the opposite relationship, studying the impact of being arrested and incarcerated before finishing school on the probability of graduating high school and reported a strong negative association.

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